June 17, 1950.

Dr. Tom C. Nelson,
181 Claremont Avenue,
New York 27, N.Y.

Dear Tom-

Of course you can show Ryan my comments. But I saw Kim Atwood last weekend (here for a conference), and I got the impression from him that the committee had already raised most of the same questions during your defense of the thesis. Does their approval of the thesis, as a whole, rigidly prevent you from modifying the publication along the lines of their discussion? Personally, I don't think that you can afford to submit it in its present form. One can argue about some of the hypothetical questions, but there are some concrete points that should certainly be amended: e.g., those mentioned under pp. ___ in my last letter:
1,23 ,24 !, table 1, and "General: c2.

I don't quite know how to interpret your letter. It sounds as if you have appreciated and agreed with most of the points raised, but think that Francis might not be reasonable about endorsing such changes. I can understand that he may have been rather preoccupied with getting ready to leave on his wonderful trip, but I can't believe that he won't be quite rational about reasonable changes, or even some small unreasonable ones, if you want them.

Have you thought through whether shaking the tubes will increase the number of collisions? It might well be likely to, but the suggestion of calculation on the basis of a temperature equivalent to the increased kinetic energy of shaking sets only an upper limit, because only a small fraction of this kinetic energy is converted into small, random motions (i.e. of particles relative to each other.) I have quickly become confused. But at least your ratio, 5300, is a lower limit, and is still comfortably large.

Your observation on gravitational separation is most interesting, but may tend to reinforce the idea that agglutination, rather than specific prototroph formation is what you have been studying. In a sense, however, there is not much difference, but the former leaves open [to the diehards] the possibility of transformation between proximate cells in the agar.

We are leaving next Friday, but won't be in Berkeley until about Agust 1 We should be in Pasadena around July 20-23 ± , and hope to see you there.

Sincerely,

Joshua Lederberg
Associate Professor of Genetics